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DOI:
10.1086/676569

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Document Version
Publisher's PDF, also known as Version of record

Citation for published version (Harvard):
Heggie, V 2014, 'Why isn't exploration a science?', *Isis*, vol. 105, no. 2, pp. 318-334.
https://doi.org/10.1086/676569

Link to publication on Research at Birmingham portal

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Why Isn't Exploration a Science?
Author(s): Vanessa Heggie
Source: *Isis*, Vol. 105, No. 2 (June 2014), pp. 318-334
Published by: The University of Chicago Press on behalf of The History of Science Society
Stable URL: http://www.jstor.org/stable/10.1086/676569
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Why Isn't Exploration a Science?

*By Vanessa Heggie**

ABSTRACT

Historians of twentieth-century science have been systematically ignoring some of the subject's richest sources and most exciting stories; this has left us with a body of work that is necessarily lopsided and that can be self-reinforcing in its insistence on certain features of "modern" science as uniquely dominant or significant after 1900. Methods, concepts, research questions, research areas, and resources that have been routinely and productively used by historians of science (and of medicine) immersed in earlier centuries appear to drop out of our toolkits when we turn to the twentieth century. This essay highlights one neglected area—human physiology studied in the field—and points to other topics where asking questions appropriate to natural history or "museum" ways of knowing might cast a completely new light on scientific practices and knowledge production.

FOR SOME HISTORIANS OF SCIENCE the question posed in this essay's title would make no sense at all. Exploration is clearly a constitutive part of scientific practice, if not a science in its own right, and the trend to global history (or at least to the Pacific and Atlantic Worlds) in the history of science has made it a point of faith that Isaac Newton is nothing without his networks, that James Cook played a crucial role in the construction of eighteenth-century astronomy, and that many parts of the Victorian scientific enterprise relied on colonial expansion, voyages in the *Beagle*, and the *Challenger* expeditions.[1] Yet

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Support for this research was provided by the Wellcome Trust (grant no. 088204/Z/09/A) and the Isaac Newton Trust, Cambridge University. Useful feedback was provided by, among others, John Pickstone, Simon Schaffer, and Bruno Strasser, as well as the audience at my 2011 "Cabinet" Garden Party Talk in Cambridge. Thanks should also go to the *Isis* referees for supportive and valuable comments and suggestions for further reading and to Lucy Martin at the Scott Polar Research Institute for help with Figure 1. The article is dedicated to John Pickstone, who, sadly, died in early 2014.

[1] For good examples see Simon Schaffer, "The Information Order of Isaac Newton's *Principia Mathematica*," Hans Rausing Lecture, Uppsala Univ., 2008; Londa Schiebinger, *Plants and Empire: Colonial Bioprospecting*

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few such narratives exist for the twentieth century. The history of twentieth-century science is routinely written about (and taught) without much consideration of nonlaboratory sciences, and the role of extraordinary encounters between human bodies and the earthly environment is rarely discussed.[2]

Robert Kohler and Henrika Kuklick pointed to a clear lacuna in the historical study of science by drawing our attention to the phenomena of modern field science, first in a special volume of *Osiris* in 1996 and then in subsequent publications.[3] This generated or foreshadowed a significant body of work on field sciences in the twentieth century, which has been supported by a distinct "spatial turn" in the history of science: several crucial publications in the 1990s considered the location—from nation to village to specific benchtop—of scientific practice.[4] But still the majority of material on the outdoor spaces of science does not venture much beyond 1900, and the work that does is usually about North American science and comes particularly from within environmental history, focusing on oceanography, climate, and geophysics. Gary Kroll, for example, has very successfully used the American notion of a "frontier" as a way to understand oceanographic research in the twentieth century; this interpretation has huge ramifications, as it is through this metaphor, he argues, that the exploitation of the ocean and the destruction of its ecosystems becomes possible.[5]

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in the Atlantic World (Cambridge, Mass.: Harvard Univ. Press, 2004); Harold L. Burstyn, "'Big Science' in Victorian Britain: The *Challenger* Expedition (1872–6) and Its *Report* (1881–95)," in *Understanding the Oceans: A Century of Ocean Exploration*, ed. Margaret Deacon, Tony Rice, and Colin Summerhayes (Boca Raton, Fla.: CRC, 2002), pp. 49–55; and Richard Sorrenson, "The Ship as a Scientific Instrument in the Eighteenth Century," *Osiris*, 1996, N.S., *11*:221–236.

[2] While space science is a notable exception here, many historians still seem more interested in the processes of travel—getting into space—or simulations than in the knowledge produced during the relatively few successful manned trips into that extraordinary nonearthly environment.

[3] Henrika Kuklick and Robert E. Kohler, "Introduction," in *Science in the Field*, *Osiris*, 1996, N.S., *11*:1–16; Kohler, "Place and Practice in Field Biology," *History of Science*, 2002, *40*:189–201; Kohler, "Labscapes: Naturalizing the Lab," *ibid.*, pp. 473–501; Kohler, *Landscapes and Labscapes: Exploring the Lab–Field Border in Biology* (Chicago: Univ. Chicago Press, 2002); and Kohler, "Finders, Keepers: Collecting Sciences and Collecting Practice," *Hist. Sci.,* 2007, *45*:1–27. See also Graeme Gooday, "Placing or Replacing the Laboratory in the History of Science?" *Isis*, 2008, *99*:783–795.

[4] Of particular relevance are Adi Ophir and Steven Shapin, "The Place of Knowledge: A Methodological Survey," *Science in Context*, 1991, *4*:3–21; David N. Livingstone, "The Spaces of Knowledge: Contributions towards a Historical Geography of Science," *Environment and Planning D: Society and Space*, 1995, *13*:5–34; Shapin, "Placing the View from Nowhere: Historical and Sociological Problems in the Location of Science," *Transactions of the Institute of British Geographers*, 1998, *23*:5–12; Crosbie Smith and Jon Agar, eds., *Making Space for Science: Territorial Themes in the Shaping of Knowledge* (London: Palgrave Macmillan, 1998); and Simon Naylor and James R. Ryan, eds., *New Spaces of Exploration: Geographies of Discovery in the Twentieth Century* (London: Tauris, 2010).

[5] Gary Kroll, *America's Ocean Wilderness: A Cultural History of Twentieth-Century Exploration* (Lawrence: Univ. Press Kansas, 2008). See also *Earth Sciences in the Cold War*, *Social Studies of Science*, 2003, *33*(5); Jacob Darwin Hamblin, *Oceanographers and the Cold War: Disciplines of Marine Science* (Seattle: Univ. Washington Press, 2005); Simone Turchetti, Simon Naylor, Katrina Dean, and Martin Siegert, "On Thick Ice: Scientific Internationalism and Antarctic Affairs, 1957–1980," *History and Technology*, 2008, *24*:351–376; Jeremy Vetter, "Rocky Mountain High Science: Teaching, Research, and Nature at Field Stations," in *Knowing Global Environments: New Historical Perspectives on the Field Sciences*, ed. Vetter (New Brunswick, N.J.: Rutgers Univ. Press, 2011), pp. 108–134; Mark V. Barrow, "On the Trail of the Ivory Bill: Field Science, Local Knowledge, and the Struggle to Save Endangered Species," *ibid.*, pp. 135–161; Helen M. Rozwadowski, "Playing By—and On and Under—the Sea: The Importance of Play for Knowing the Ocean," *ibid.*, pp. 162–189; James Rodger Fleming, "Planetary-Scale Fieldwork: Harry Wexler on the Possibilities of Ozone Depletion and Climate Control," *ibid.*, pp. 190–211; Elena Aronova, Karen S. Baker, and Naomi Oreskes, "Big Science and Big Data in Biology: From the International Geophysical Year through the International Biological Program to the Long Term Ecological Research (LTER) Network, 1957–Present," *Historical Studies in the Natural Sciences*, 2010, *40*:183–224; and Kristian Hvidtfelt Nielsen, "Postcolonial Partnerships: Deep Sea Research, Media Coverage, and (Inter)national Narratives on the Galathea Deep Sea Expedition from 1950 to 1952,"

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Other historians have used oceanography and geophysical sciences to describe how supposedly cooperative international projects were crucial sites for national rivalry and national identity formation.[6] More recent work has begun to show the importance of exploration (and particularly anthropological expeditions) to the science of heredity, race, and genetics in the last century.[7] But this material is dwarfed by the vast number of works considering exploration and field science in the eighteenth or nineteenth centuries. Even in the more focused texts examples rarely come from the twentieth century; of all David Livingstone's examples of "the spaces of science," it is only "the body" that is illustrated with any twentieth-century materials (the Pill and human experiments conducted by the Nazis).[8]

In addition, while a few books discuss field science in the twentieth century rather than telling the more common story of big science in laboratories, that institution is still essentially present in much of this writing. Taking a frame that opposes the laboratory and the field and interposes a "boundary" between them means that work located in the field still tends to be defined according to its resemblance to, or deviation from, controlled internal spaces. In these stories particular experimental practices—often standardization and metrology—are not interpreted as perfectly normal forms of scientific activity but, rather, are presented as intrinsically the property of the laboratory. These activities are therefore read as attempts to discipline and thus denature the outdoors, rendering it laboratory-like rather than field-like. Consequently, the field scientist's activities are interpretable only as either a form of resistance against or as capitulation to a more

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British Journal for the History of Science, 2010, *43*:75–98. For a particular take on geophysical and climate science and the Cold War see the products of the European Research Council–funded project "The Earth under Surveillance: Climate Change, Geophysics, and the Cold War Legacy," http://teus.unistra.fr/ (accessed July 2012).

[6] Nielsen, "Postcolonial Partnerships"; and Katrina Dean, Simon Naylor, Simone Turchetti, and Martin Siegert, "Data in Antarctic Science and Politics," *Soc. Stud. Sci.*, 2008, *38*:571–604.

[7] See, e.g., Warwick Anderson, "Hybridity, Race, and Science: The Voyage of the *Zaca*, 1934–1935," *Isis*, 2012, *103*:229–253. A particularly convincing example of the value of this approach was "Making Human Heredity: Populations and Public Health in the Postwar Era," a conference held at the University of Cambridge in June 2012. The papers are forthcoming in a special issue of *Studies in History and Philosophy of Science*; in particular see work by Jenny Bangham, Veronika Lipphardt, Susanne Bauer, Joanna Radin, and Alexandra Widmer.

[8] David N. Livingstone, *Putting Science in Its Place: Geographies of Scientific Knowledge* (Chicago: Univ. Chicago Press, 2003). The literature for the period prior to 1900 is enormous. As well as the material cited in note 1, above, I have found the following particularly useful: Simon Schaffer, *From Physics to Anthropology and Back Again* (Cambridge: Prickly Pear, 1994); Bruce Hevly, "The Heroic Science of Glacier Motion," *Osiris*, 1996, N.S., *11*:66–86; Dorinda Outram, "On Being Perseus: New Knowledge, Dislocation, and Enlightenment Exploration," in *Geography and Enlightenment*, ed. Livingstone and Charles W. J. Withers (Chicago: Univ. Chicago Press, 1997), pp. 281–294; Felix Driver, *Geography Militant: Cultures of Exploration and Empire* (Oxford: Blackwell, 2001); Livingstone, *Putting Science in Its Place*; Michael Dettelbach, "The Stimulations of Travel: Humboldt's Physiological Construction of the Tropics," in *Tropical Visions in an Age of Empire*, ed. Driver and Luciana Martins (Chicago: Univ. Chicago Press, 2005), pp. 42–58; David Arnold, "Envisioning the Tropics: Joseph Hooker in India and the Himalayas, 1848–1850," *ibid.*, pp. 211–234; A. L. Rice, "Discovery at Sea: A Heady Mix of Scientists, Ships, and Sailors," *Archives of Natural History*, 2005, *32*:177–191; Michael F. Robinson, *The Coldest Crucible: Arctic Exploration and American Culture* (Chicago: Univ. Chicago Press, 2006); Thomas F. Gieryn, "City as Truth-Spot: Laboratories and Field-Sites in Urban Studies," *Soc. Stud. Sci.*, 2006, *36*:5–38; Jim Endersby, *Imperial Nature: Joseph Hooker and the Practices of Victorian Science* (Chicago: Univ. Chicago Press, 2008); Philipp Felsch, "Mountains of Sublimity, Mountains of Fatigue: Towards a History of Speechlessness in the Alps," *Sci. Context*, 2009, *22*:341–364; Stéphane Le Gars and David Aubin, "The Elusive Placelessness of the Mont-Blanc Observatory (1893–1909): The Social Underpinnings of High-Altitude Observation," *ibid.*, pp. 509–531; and Hannah Hodacs, "Linnaeans Outdoors: The Transformative Role of Studying Nature 'On the Move' and Outside," *Brit. J. Hist. Sci.*, 2011, *44*:183–209.

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powerful mode of scientific practice.[9] This approach is unhelpful, as it can conflate the location of scientific practice ("field" here meaning something quite literal—the world outdoors) with the content of that scientific practice.[10] In contrast, work dealing with the same conflict in the eighteenth and nineteenth centuries has shown clearly that the debate was in fact a more rhetorical and nuanced comparison of the armchair savant and the adventurer—that is, a debate about personality and reputation as much as about practice.[11]

It is useful here to consider John Pickstone's "ways of knowing." As a recent special edition of *History of Science* reminds us, Pickstone is one of the few historians of science who has attempted systematically to order styles and modes of scientific practice in the modern era.[12] This approach has evolved over two decades, but roughly speaking four ways of knowing and doing are identified: world reading and rhetoric; natural history and craft; analysis and rationalization; and synthetic experimentalism and technology. Although Pickstone is emphatic that these ways of knowing and of doing not only coexist but are also combined, so that science is always a heterogeneous practice, it is clear that the latter two categories—that is, the analysis and rationalization of laboratory sciences and the synthetic "technosciences"—dominate our accounts of late nineteenth- and twentieth-century science.

Pickstone does write of natural history and craft, nonexperimental analysis, and rhetoric as parts of twentieth- and twenty-first-century science, particularly in fields like molecular biology. This interpretation has been examined by Bruno Strasser and Soraya de Chadarevian, who also find natural historical and analytical ways of knowing present in modern science but criticize Pickstone's model because "calling a way of knowing 'natural historical' somewhat defeats the purpose of the category which is to allow for the exploration of different ways of knowing within disciplines, including natural history."[13] I want to make a similar double argument for the value of "field science" as a topic for historical study and the simultaneous unhelpfulness of "field science" as a category of practice when it is used to isolate or to "other" some kinds of scientific activity.

This essay therefore proposes two ways to reform our current approach to the history of recent science. First, it argues that we need a much better understanding of these field sciences in their modern form—and this will include reading them on their own terms rather than in opposition to the laboratory. Most dramatically, this may require us to reconsider our definition of modern scientific practice, and it will at the very least require us to reprioritize areas of science that have so far been neglected. Second, I want to suggest that we should never assume a hierarchy of practices or of sites; collecting, observing, experimenting, ordering, standardizing, and physically experiencing the world have all, at times, been normal scientific practices that produce reliable knowledge about

[9] Simon Naylor, "Fieldwork and the Geographical Career: T. Griffith Taylor and the Exploration of Australia," in *New Spaces of Exploration*, ed. Naylor and Ryan (cit. n. 4), pp. 105–124.

[10] For a similar argument see Gregg Mitman's review of Kohler, *Landscapes and Labscapes*, *Journal of the History of Biology*, 2003, *36*:599–601; for Kohler's response see Robert E. Kohler, "History of Field Science: Trends and Prospects," in *Knowing Global Environments*, ed. Vetter (cit. n. 5), pp. 212–240.

[11] Outram, "On Being Perseus" (cit. n. 8); and Driver, *Geography Militant* (cit. n. 8), pp. 11–20.

[12] See *Natural Histories, Analyses, and Experimentation: Dissecting the Working Knowledges of Chemistry, Medicine, and Biology since 1750*, *Hist. Sci.*, 2011, *49*(3). For Pickstone's work see John V. Pickstone, "Ways of Knowing: Towards a Historical Sociology of Science, Technology, and Medicine," *Brit. J. Hist. Sci.*, 1993, *26*:433–458; and Pickstone, *Ways of Knowing: A New History of Science, Technology, and Medicine* (Chicago: Univ. Chicago Press, 2001). See also Pickstone, "Museological Science? The Place of the Analytical/Comparative in Nineteenth-Century Science, Technology, and Medicine," *Hist. Sci.*, 1994, *32*:111–138.

[13] Bruno J. Strasser and Soraya de Chadarevian, "The Comparative and the Exemplary: Revisiting the Early History of Molecular Biology," *Hist. Sci.*, 2011, *49*:317–336, on p. 319.

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the world and that can be deployed in—not dictated by—multiple sites of scientific activity.[14]

The promise of this approach can be shown most clearly through examples; these demonstrate that the sorts of questions we ask of, and the sorts of sources and practices we routinely and productively consider for, the period before 1900 need to be systematically redeployed (with appropriate adjustments) to help us better understand the twentieth century. To make clear the blind spot in the modern history of science, take this experiment: In July 1911 three British men embarked on an exciting piece of physiological research—a study of metabolism, exercise, and nutrition. This was self-experimentation, as was typical for human physiology at the time, so all three experimenters were also experimental subjects. The question they were tackling was a fundamental one: they were trying to establish the ideal ratio between proteins, fats, and carbohydrates in the diet of men doing heavy exercise (or heavy manual labor). Since Justus von Liebig's 1843 assertion in *Animal Chemistry* that it was the nitrogenous parts of the diet that were "nutritive," while all other parts were merely "respiratory," there had been a rolling debate among physiologists about the source of energy for the resting and exercising muscle. On the one side was a hypothesis that it was the active breakdown of muscle tissue itself that released the energy required for movements, and so it was protein that was the direct source of bodily energy; on the other side were a range of hypotheses suggesting that either carbohydrates or fats, or combinations of all the food groups, were broken down to release chemical energy (with or without the synthesis of intermediate chemical forms).[15]

This experiment was to be unique in the history of metabolic studies. Each of the three participants was allocated a diet high in one food factor—fat, carbohydrate, or protein—and then asked to do extraordinarily intense endurance exercise over several weeks (most comparable experiments only lasted a few hours to a couple of days). Further, each experimenter was to self-regulate his diet; they gradually altered the proportions of the three food groups according to what they intuitively sensed they needed and according to their physical performance compared to that of their colleagues. Thus each experimenter was also to act as a control. Although no chemical analyses were made on the bodies of the experimenters, the nutritional content of the food was known and the experiment was rigorously conducted, since the men were under each other's observation for twenty-four hours a day, with no possibility of exaggerating or avoiding exercise or eating any food other than that provided for the experiment. The conditions, particularly the weather, that affected the exercise portion of the experiment were carefully noted. The result was a perfect agreement on the necessary diet: all three men came to almost exactly the same ratio of three parts (by weight) of protein to four of carbohydrate and one of fat.

While the lack of on-the-spot chemical analyses meant that this work could not answer some of the biochemical questions about metabolism, the experience of the subjects and

[14] It is interesting to compare the possible invention of the field/lab divide, or the observation/experiment conflict, to recent work in the history of medicine that reconsiders the art/science divide in clinical medicine. See Steve Sturdy, "Looking for Trouble: Medical Science and Clinical Practice in the Historiography of Modern Medicine," *Social History of Medicine*, 2011, *24*:739–757.

[15] See Kenneth J. Carpenter, *Protein and Energy: A Study of Changing Ideas in Nutrition* (Cambridge: Cambridge Univ. Press, 1994), for a review. For the originals see F. W. Pavy, "The Effect of Prolonged Muscular Exercise on the System," *Lancet*, 1876, *107*:319–320, 353–356, 392–394, 429–431, 466–468; A. Fick and J. Wisclicenus, "On the Origin of Muscular Power," *Philosophical Magazine and Journal of Science (London)*, 1866, *31*:485–503; and Carl von Voit, *Untersuchungen über den Einfluss des Koschsalzes, des Keffee's und der Musckelbewegungen auf den Stoffwechsel* (Munich: Literarisch-Artistische Anstalt, 1860).

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Figure 1. *June 1911. Three members of the 1911–1913 "Terra Nova" expedition carefully weigh out sledge rations, including pemmican (tube-shaped, far left). Left to right: Dr. Edward Atkinson, Lieutenant Henry Bowers, and Mr. Apsley Cherry-Garrard. Photographer: Herbert Ponting. Image © Scott Polar Research Institute, University of Cambridge; reproduced here with permission.*

their convergence on a single dietary regime certainly showed the priority of carbohydrate over protein, significantly downplayed the role of fat, and pointed in a clear research direction (as well as providing model dietaries of immediate practical value to explorers and soldiers). Yet a historian cannot find out about this experiment by reading scientific journals or medical texts. This is not because the studies vanished into obscurity—in fact, these three experimenters were simultaneously conducting two other distinct scientific projects at the same time as their metabolic research, and at least one of these is celebrated as one of the most bittersweet scientific endeavors of the twentieth century. These dietary studies were conducted by Henry Bowers, Apsley Cherry-Garrard, and Bill Wilson on their 130-mile round-trip to collect Emperor Penguin eggs from Cape Crozier in the Antarctic winter of 1911, later known as the "Worst Journey in the World." (See Figure 1.)

The fact that the early twentieth-century British expeditions to the South Pole were deeply scientific in character, purpose, and personnel has been recognized before—most notably by the British historian Max Jones in his seminal *The Last Great Quest* and most recently by Edward Larson in his *Empire of Ice*. But neither of these books gives much space to the physiological work done. Further, Gordon Elliott Fogg's otherwise magisterial *History of Antarctic Science* explicitly states that "planned medical studies with wintering parties began with the US Antarctic Service expedition of 1939–41" and that although earlier expeditions "took medical men, of course[,] . . . [and] systematic recording of such things as men's weights, measurements, pulse rates and blood counts was done

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on some expeditions . . . there was little that could be dignified by the name of medical research."[16] This does not seem a fair evaluation when we take the physiological work done in July 1911 on its own merits: the threefold purpose of the "Worst Journey in the World" was to collect penguin eggs, take crucial meteorological records, and design the perfect *scientific* sledging ration for the attempt on the Pole later in the year. The rations were to be determined through the experimental use of human bodies in a rationalized field test of foods that had already been subject to nutritional analysis and careful logistical planning. The design and organization of the experiment is certainly on a par with much work published in the *American Journal of Physiology* or notes in the *Lancet* in the early years of the twentieth century. The intention, we can assume on the basis of precedent, was also to pass on the conclusions reached through publications, not just for the edification of future explorers, but also for the wider audience of scientists and military strategists interested in the performance of the human body.[17]

Every generation of historians of science has its "What is science?" debate, and concretizing that definition is not the primary aim of this essay; nonetheless, it does seem hard to make a case that this research in Antarctica was *not* science, when considered in context. So why has it been ignored? There are two mutually reinforcing reasons. One is that this sort of science (field-based, whole-body physiology) has not received much scholarly attention. The second is that in order to find out about this event we have to read Cherry-Garrard's *Worst Journey in the World*.[18] The nature of the source exacerbates the neglect of the topic: Cherry-Garrard's book does not look like a typical source for the history of twentieth-century science. And this is strange, for the book is *exactly* the sort of source that historians of earlier centuries use avidly and, more to the point, *exactly* the sort of source that early twentieth-century scientists, explorers, and enthusiastic amateurs used to learn scientific facts about their bodies and the world around them. It was not unreasonable for Robert Falcon Scott and his teams to expect that their stories, published as popular exploratory accounts, would be as valid and perhaps more valuable as sources of facts, including scientific facts, as a paper in a scientific or medical journal.[19]

Scott himself is explicit about the value of explorers' first-person accounts. For his first trip to the far South in 1901–1904 the Royal Geographical Society (RGS) provided him with *The Antarctic Manual for the Use of the Expedition of 1901*, which had some significant flaws:

> It is only when one comes to make the attempt [to follow the instructions] that one finds that a simple experiment is almost an impossibility; every condition is complicated by outside variable causes. . . . It was suggested in our Antarctic Manual of Instructions that a block of ice should be suspended in the sea and its rate of increase in winter and decrease in summer should be measured. Had we attempted to do this, probably we should have arrived at an utterly false conclusion because in no two places would the result be the same. . . . It shows the impossibility

[16] Max Jones, *The Last Great Quest: Captain Scott's Antarctic Sacrifice* (Oxford: Oxford Univ. Press, 2003); Edward J. Larson, *An Empire of Ice: Scott, Shackleton, and the Heroic Age of Antarctic Science* (New Haven, Conn.: Yale Univ. Press, 2011); and G. E. Fogg, *A History of Antarctic Science* (Cambridge: Cambridge Univ. Press, 1992), p. 377.

[17] Elizabeth Baigent, "'Deeds Not Words'? Life Writing and Early Twentieth-Century British Polar Exploration," in *New Spaces of Exploration*, ed. Naylor and Ryan (cit. n. 4), pp. 23–51.

[18] Apsley Cherry-Garrard, *The Worst Journey in the World: Antarctic, 1910–1913* (London: Constable, 1922).

[19] This is a trend discussed in much more detail for the period before 1900 in Driver, *Geography Militant* (cit. n. 8), esp. Ch. 3: "Hints to Travellers: Observation in the Field."

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> of carrying out experiments of this sort, however easily they may be conceived in the quiet of an English study.[20]

Scott showed a clear preference for the experienced traveler's account over the advice of the armchair scientist. Even the "crowdsourced" *Hints to Travellers*, which was also produced by the RGS, was not of much more use, as it was not until 1938 that the society managed to add significant Antarctic information (based on the reports of traveling scientists and explorers). In these travelers' accounts Scott still found deficiencies, often a lack of necessary technical or scientific details. Consequently, he seems to have found most useful the opportunity to meet with explorers and, crucially, conduct his own experiments and tests in the Antarctic. So the scientific inputs and outputs of polar exploration were not necessarily written down—and they were rarely written down in the more commonly consulted twentieth-century scientific sources.

Century-old metabolic research in the far South may seem very far removed from the interests of most historians of modern science and medicine, but the point about neglected sources applies strongly for other research in other times and quite other places. John Hunt's *The Ascent of Everest* is a piece of classic writing in exploration and mountaineering literature and has rarely been out of print in one form or another, including a special edition in 2003 to celebrate the fiftieth anniversary of the first successful attempt on the summit of the world's highest mountain.[21] Hunt's book is easily recognized by historians and sociologists as a valuable source illustrating notions of imperial masculine identity, of British national identity—particularly the celebration of grit and pluck against the odds—and of 1950s ideas about man's relationship to nature.[22] It is less easily recognized as a scientific text, and to my knowledge it has never been analyzed as such. Yet the full editions of *The Ascent of Everest* contain extensive appendixes (including diagrams) on the technical equipment used—radios, first-aid boxes, high-tech clothing—on the oxygen systems, on diet, on physiology, and on the medical demands of a high-altitude visit. Here, and in the text itself, previous scientific work is discussed and evaluated against the field experiences with gas regulators, windproof stoves, and frostbite.

Perhaps even more important, the book is a crucial and unique source of insight into the reality of experimental practice. While seminal texts in the history and sociology of twentieth-century science concern themselves with close readings of laboratory notebooks, accounts of twentieth-century scientific practice have been hidden in plain sight in the books of expedition leaders and mountaineers.[23] To take one example, this is the physiologist L. G. C. E. Pugh's account of maximum work tests done on the Everest team in 1953, as published in the *Journal of Physiology*: "Maximum oxygen consumption and ventilation during short periods of exhausting exercise were studied on the expedition to

[20] R. F. Scott, *The Voyage of the* Discovery (1905; London: Wordsworth, 2009), pp. 214–215.

[21] My quotations are taken from John Hunt, *The Ascent of Everest*, 1st ed. (London: Hodder & Stoughton, 1953).

[22] For an examination of some of these issues see the debate between Gordon Stewart and Peter Hansen: Gordon Stewart, "Tenzing's Two Wrist-Watches: The Conquest of Everest and Late Imperial Culture in Britain, 1921–1953," *Past and Present*, 1995, *149*:170–197; and Peter H. Hansen, "Debate: Tenzing's Two Wrist-Watches," *ibid.*, 1997, *157*:159–177.

[23] For work focusing on close reading of laboratory notebooks see Allan D. Franklin, "Millikan's Published and Unpublished Data on Oil Drops," *Historical Studies in the Physical Sciences*, 1981, *11*:185–201. See also the introduction to Frederick L. Holmes, Jürgen Renn, and Hans-Jörg Rheinberger, eds., *Reworking the Bench: Research Notebooks in the History of Science* (Dordrecht: Kluwer, 2003), pp. vii–xv; and Holmes, "Laboratory Notebooks and Investigative Pathways," *ibid.*, pp. 295–308.

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Mount Everest. Experiments were done on MPW, CW and GP between 7000 and 8000 ft during the approach march; on MPW, EPH and three Sherpas at the main base [13,000 ft] . . . and on MPW, TS and GP in the West Cwm on Mount Everest at either 20,500 ft. or 21,200 ft." The article gives no explanation for the inconsistencies in the experimental subjects studied. On the other hand, *The Ascent of Everest* offers some insight into the rigors of experimental subject selection:

> There had been sacrifices to science which I was glad to avoid. Griff Pugh had subjected the party to a fearful ordeal known as a "maximum work test," consisting of running uphill at best possible speed until the lungs were bursting and then expiring air into an enormous bag until it swelled out like a balloon. It was satisfactory to learn that Griff . . . had not spared himself the tortures which he inflicted on his guinea-pigs. We hurried forward to catch up the others lest we should be tested in our turn.[24]

Mountaineering books do not just describe scientists and their practices from novel angles—they also discuss a host of otherwise invisible "supporting staff." Again, it is the case that for the sixteenth through nineteenth centuries the importance and significance of technicians, demonstrators, assistants, family members, indigenous informers, pundits, and "native guides" have been recently, and rightly, emphasized. Likewise, studies exploring the concept of the "backstage" have proved invaluable for enriching our understanding of historical scientific practices.[25] Such approaches would surely prove as useful for the twentieth century. Some of the most exciting work relating to pundits, by Kapil Raj, specifically discusses how "human travellers become instruments," how their bodies and labor are organized for imperial purposes; in light of this work, what are we to make of this 1944 discussion of the use and management of Sherpa guides by the controversial British climber Eric Shipton?

> Many expeditions coming out from Europe, have sent word to Darjeeling to order so many Sherpas to be sent to meet them . . . just as one might order pack mules. Often they have received very inferior material. . . . The Sherpas are essentially individualists, and they do not take kindly to mechanical discipline. Mrs Townsend, the Secretary of the Eastern Section of the Himalayan Club, has done a great deal towards the organisation of the Sherpas. Every man who has taken part in serious expeditions has been given a little book in which is entered his past record together with a note written by each of his employers.[26]

The Sherpa have been crucial, and thus far neglected, participants in twentieth-century research—in the physical, chemical, environmental, geographical, *and* biological sciences—at very least as porters, carrying fragile equipment to altitude, and on occasion actively taking measurements or acting as research objects for physiologists and geneticists.[27] There is

[24] L. G. C. E. Pugh, "Muscular Exercise on Mount Everest," *Journal of Physiology*, 1958, *141*:233–261, on p. 236; and Hunt, *Ascent of Everest* (cit. n. 21), p. 71.

[25] See, e.g., Simon Schaffer, "Experimenters' Techniques, Dyers' Hands, and the Electric Planetarium," *Isis*, 1997, *88*:456–483; and Stephen Pumfrey, "Who Did the Work? Experimental Philosophers and Public Demonstrators in Augustan England," *Brit. J. Hist. Sci.*, 1995, *28*:131–156.

[26] Kapil Raj, "When Human Travellers Become Instruments: The Indo-British Explorations of Central Asia in the Nineteenth Century," in *Instruments, Travel, and Science: Itineraries of Precision from the Seventeenth to the Twentieth Century*, ed. Marie-Noëlle Bourquet, Christian Licoppe, and H. Otto Sibum (London: Routledge, 2002), pp. 156–188; Raj, *Relocating Modern Science: Circulation and the Construction of Knowledge in South Asia and Europe, 1650–1900* (New York: Palgrave Macmillan, 2007); and Eric Shipton, *Upon That Mountain* (London: Hodder & Stoughton, 1944), p. 148.

[27] Sherry Ortner's excellent anthropological work is a rare exception to this neglect. See Sherry B. Ortner,

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much worth studying in the lives and work of nonindigenous peoples, too: the military personnel whose assistance is crucial for polar work or the explorers, sportspeople, and mountaineers who organize, enable, and participate in scientific expeditions and field experiments. Often these participants have complicated identities, being scientists *and* mountaineers *and* military personnel *and* indigenous representatives in various combinations.

As Simone Turchetti and others have demonstrated, when it comes to the physical sciences in remote geographical locations the conflicts between the desire to share data and the need for scientific national security are real and consequential.[28] Similar questions ought to be asked of the medical and biological sciences. In an only occasionally less significant way, issues of sporting and exploratory prestige have also affected the ways in which information—routes, diets, radio technology, altitude-proof ovens, new sports drugs—is shared between and within nations. There is an international trade in this knowledge and information, at least some of which comes from exploration and sporting literature rather than the scientific press. One of the most technical books about oxygen systems, respiration, and high-altitude climbing from the first half of the twentieth century is George Ingle Finch's *The Struggle for Everest*, an account of the 1922 British expedition to Everest. Finch was a chemist, appointed a Fellow of the Royal Society in 1938 and awarded that society's Hughes Medal in 1944 for his research. It is consequently not surprising that a great deal of his book is given over to science—that is, physiology, diet, barometric pressures, and oxygen technologies. But Finch had a major disagreement with the aristocracy of the British climbing world, and, perhaps as a consequence, his book was not printed in English until 2008.[29] Its original language was German: *Der Kampf um den Everest* was published in 1925.

By the early decades of the twentieth century Germany had developed a strong tradition of alpine climbing, and this extended to the Himalayas by the 1930s, where Nanga Parbat became the German "mountain of destiny" and the site of disasters and triumphs as dramatic as anything Britons were doing on Everest.[30] Many physiologists with an interest in mountaineering found their way into military research, particularly in the facilities of the Luftwaffe, as technologies from mountaineering—especially those relating to altitude acclimatization, cold resistance, and oxygen supply systems—were found relevant to aviation. In turn, some of these physiologists were removed to the United States via Operation Paperclip to work on aviation, rocket, and space technology in the 1950s and 1960s.[31] So a book about mountaineering, like Finch's *Der Kampf um den Everest*, should properly be considered as a scientific and technical text, which may be part of a much

"Thick Resistance: Death and the Cultural Construction of Agency in Himalayan Mountaineering," *Representations*, 1997, *59*:135–162; and Ortner, *Life and Death on Mt. Everest: Sherpas and Himalayan Mountaineering* (Princeton, N.J.: Princeton Univ. Press, 1999).

[28] Turchetti *et al.*, "On Thick Ice" (cit. n. 5); Dean *et al.*, "Data in Antarctic Science and Politics" (cit. n. 6); and Simone Turchetti, Katrina Dean, Simon Naylor, and Martin Siegert, "Accidents and Opportunities: A History of Radio Echo-Sounding of Antarctica, 1958–1979," *Brit. J. Hist. Sci.*, 2008, *41*:417–444.

[29] George W. Rodway, ed., *George Ingle Finch's "The Struggle for Everest"* (Trowbridge, Wiltshire: Carreg, 2008); and Rodway, "George Ingle Finch and the Mount Everest Expedition of 1922: Breaching the 8000-m Barrier," *High Altitude Medicine and Biology*, 2007, *8*:68–76.

[30] Lee Wallace Holt, "Mountains, Mountaineering, and Modernity: A Cultural History of German and Austrian Mountaineering, 1900–1945" (Ph.D. diss., Univ. Texas, Austin, 2008); Harald Höbusch, "Ideologizing Nanga Parbat: High-Altitude Mountaineering and German Nationalism in the 1930s," *Sport in History*, 2003, *23*:64–88; Höbusch, "Germany's Mountain of Destiny: Nanga Parbat and National Self-Representation," *International Journal of the History of Sport*, 2002, *19*:137–168; and Höbusch, "Rescuing German Alpine Tradition: Nanga Parbat and Its Visual Afterlife," *Journal of Sport History*, 2002, *29*:48–76.

[31] E.g., Ulrich Luft; see George W. Rodway, "Ulrich C. Luft and Physiology on Nanga Parbat: The Winds of War," *High Alt. Med. Biol.*, 2009, *10*:89–96.

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larger story about twentieth-century global science that includes military research and secrecy, bioethics and the Nuremberg trials, the Cold War, and the space race. Books similar to this published in the nineteenth century, or earlier, are routinely used by historians of science. Finch, and scientists like him, may not have the stature of an Alexander von Humboldt or a Francis Galton, but that fact alone does not seem to excuse us from taking the travel literature of twentieth-century science as seriously as we do that of the years before 1900. Why should we study the *Hints to Travellers* edited by Galton and not, in turn, the writings of Scott—who took (and dismissed) the *Hints*?

One answer is that perhaps scientists themselves took this literature less seriously. Certainly the modern history of geography identifies a change in the self-representation of geographers around 1900. Simon Naylor, Elizabeth Baigent, and James Ryan argue that biographical and narrative accounts of expeditions began to be seen, at least by geographers, as less academic, less serious, and less scientific than publications that followed the format that had become widespread in the other sciences. In this account, geography became respectable—the Royal Geographic Society became (or remained) scientific—through a repudiation of heroic explorer narratives and a deliberate prioritization of academic geography. Historians have also identified a similar process within anthropology—that is, a *rhetorically deployed* divide between natural historical methods and "scientific" organized survey work.[32] But as this process is about self-representation and rhetoric, it must be clearly read as such. As an attitude it is temporary, never hegemonic, and in no sense inevitably representative of changes *in practice*.[33] Take the example of Pugh's physiological research on Everest, quoted above; Pugh clearly had no difficulty writing this work as a story suitable for acceptance in the *Physiological Journal*, with anonymized "guinea-pigs" and tidy data. But this is only one available representation of his experience, and there is no good reason for us to prioritize it as a historical source over the account given in Pugh's diaries or that offered by Hunt.[34] We can see that explorers like Hunt and Scott used travel narratives as sources of scientific and technical knowledge; we find that professional scientists such as Finch published their research in a variety of formats.

If we have been misled by considering too closely scientists' own hagiographic accounts of what makes twentieth-century science special, or their apparent "molecular biology envy," then our error has extended into every science, a point concretely made by Bruno Strasser's recent work on DNA databases—that is, on molecular biology itself. Molecular biology is a dominant topic in the history of science in the twentieth century—and rightly so—but what Strasser's work elegantly demonstrates is how much of the story

[32] See Naylor and Ryan, eds., *New Spaces of Exploration* (cit. n. 4), particularly Simon Naylor and James R. Ryan, "Exploration and the Twentieth Century," *ibid.*, pp. 1–22; Baigent, "'Deeds Not Words'?" (cit. n. 17); and Naylor, "Fieldwork and the Geographical Career" (cit. n. 9). On the change in the self-representation of geographers ca. 1900 see Peter Collier and Rob Inkpen, "The RGS, Exploration, and Empire and the Contested Nature of Surveying," *Area*, 2002, *34*:273–283. Regarding anthropology see James Urry, *Before Social Anthropology: Essays on the History of British Anthropology* (Studies in Anthropology and History, 6) (Chur: Harwood, 1993).

[33] See also Driver, *Geography Militant* (cit. n. 8), on the rhetorical divide between the armchair and the exploring geographer. On the clear prioritization of the "heroic" science of altitude physiology on mountains rather than in the laboratory see Vanessa Heggie, "Experimental Physiology, Everest, and Oxygen: From the Ghastly Kitchens to the Gasping Lung," *Brit. J. Hist. Sci.*, 2012, *46*:123–147; on the continuing use of physical endurance as a mark of scientific respectability see Henrika Kuklick, "Personal Equations: Reflections on the History of Fieldwork, with Special Reference to Sociocultural Anthropology," *Isis*, 2011, *102*:1–33.

[34] John West has gathered many of Pugh's scientific and personal papers in a special collection: L. G. C. E. Pugh Papers, Mandeville Special Collections Library, University of California, San Diego.

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of molecular biology we are missing by neglecting natural history, which he defines as "not . . . the study of whole organisms . . . [but] the different practices of collecting, describing, naming, comparing, and organizing natural objects, practices usually associated not with the laboratory but with the wonder cabinet, the botanical garden, or the natural history museum."[35]

Strasser's essay shows that when we forget the heterogeneity of scientific "ways of knowing" or "ways of doing," when certain approaches drop out of our historians' toolkit, we miss things. A further danger of category dominance is that we end up defining our nondominant ways of knowing as "other"—that is, framing them through what they are not: *not* technoscience, *not* experiment. Fields are not-laboratories. *The Ascent of Everest* is not-scientific writing. We are left with the assumption that the laboratory is absolutely the normative site for scientific practices, while everywhere else is a deviation or second-best option, a site whose ability to construct facts has at least to be strongly argued, while the lab remains an unproblematic source of truth. But we know that laboratories do not make knowledge so simply; the tradition in which most contemporary historians of science have been educated is one deeply informed by the work of sociologists and anthropologists who treated laboratories as field sites within their own disciplines.[36] While we have taken seriously—if only to refute—the socially constructed nature of work done in these sites, we have not taken so seriously the idea that the laboratory is merely one of many kinds of field site for science; it is a subcategory, not a superior ideal that other sites must try to match. For many scientists in the twentieth century the field has been the priority area for research, and often it is field that is the producer of truths that the laboratory only unsuccessfully and inefficiently replicates.[37]

It is possible to argue that the examples of neglected science I have used here—mostly mountaineering and polar research—might seem peripheral or fringe subjects. I believe such a view would be shortsighted: the case of the Operation Paperclip scientists makes it clear that this work can be linked to major topics in the history of modern science, not least military science, space exploration, the Cold War and international relations, medical ethics and human experimentation, and the construction of national identities.[38] Historians of oceanography and environmental/global science have made their own arguments for the value of field and exploration science.[39] So while these practices appear at first reading to be exotic, and perhaps not of much significance compared to great technoscience or laboratory-experimental projects, closer readings show otherwise. And, in any case, it is impossible to make the argument for the relative irrelevance of natural history *as a way of knowing* when it has been specifically shown to be such a useful analytical tool for the understanding of molecular biology, which is the archetypal (perhaps even pattern) technoscience for the period since the 1930s.

In fact, the projects I am promoting are embedded in some of the most critical problems

[35] Bruno J. Strasser, "The Experimenter's Museum: GenBank, Natural History, and the Moral Economies of Biomedicine," *Isis*, 2011, *102*:60–96, on p. 62. See also Strasser, "Collecting, Comparing, and Computing Sequences: The Making of Margaret O. Dayhoff's Atlas of Protein Sequence and Structure, 1954–1965," *J. Hist. Biol.*, 2009, *43*:623–660; and Strasser, "Laboratories, Museums, and the Comparative Perspective: Alan A. Boyden's Serological Taxonomy, 1925–1962," *Hist. Stud. Nat. Sci.*, 2010, *40*:533–564.

[36] Bruno Latour and Steve Woolgar, *Laboratory Life: The Construction of Scientific Facts* (Princeton, N.J.: Princeton Univ. Press, 1986).

[37] Heggie, "Experimental Physiology, Everest, and Oxygen" (cit. n. 33).

[38] Not to mention the individual technological innovations, from the development of commercial aviation, through incubators for premature babies, to the use of erythropoietin doping in modern sports.

[39] See the work cited in note 4, above.

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in contemporary history of science; in 2008 Peter Galison published a series of "Ten Problems" in the history and philosophy of science "to gesture toward some problem areas where only an intense collaboration of effort can help us move forward," and the sorts of approaches and topics discussed in this essay are clearly included in his list.[40] Galison's Problem 6 covers the "Political Technologies," one of which is bioprospecting, a portmanteau word meaning "biodiversity prospecting"—that is, taking the diversity of nature and searching it for commercially and medically useful technologies, concepts, or chemicals. Galison refers to it specifically as a puzzle for intellectual property law, and especially for privacy, in the context of genetic information, cultured cell lines, and the patenting of living things. There is excellent work on this topic. Bioprospecting is an enormous ethical challenge, dealing with the ownership of biological products—plant, animal, and human; it is frequently a process in which Western pharmaceutical or scientific organizations find source material in developing nations, with help (sometimes unacknowledged) or resistance from indigenous peoples. Anthropologists, sociologists, and bioethicists contribute to an exciting field in the history of science and medicine that focuses on bioethics, postcolonial and colonial relationships, cultural incommensurability and the reappropriation of knowledge, local resistance, and reinterpretation.[41]

And yet there are still some outstanding questions. Somewhere on the globe there are bioprospectors looking for new products, chemicals, and cures. What is their professional identity? Do these people (are they men or women?) identify as biologists, or botanists, or geneticists, or even explorers? How do they train for this work? How do they relate to laboratory chemists and systems biologists at conferences back at the company headquarters? Are they permanent staff or contractors? How does the center/periphery relationship work for twentieth-century bioprospecting? Thinking of those nineteenth-century pundits, how are relationships with local go-betweens and experts negotiated? When the bioprospector believes that he or she has successfully located a new species or a target product, who verifies the finding? What are the pathways of authority and expertise that validate bioprospectors' claims? These questions of authority are a central part of work on nineteenth-century, and earlier, botanical collectors and scientific explorers. Yet they are largely absent in studies of the twentieth century. While bioinformatics, globalization, and technological privacy are exciting and fundamental features of twentieth-century history of science, we must not lose sight of the fact that they exist in the company of, and in some cases entirely depend on, museological science and analytical ways of knowing, natural history, collectors, the field.

The failure to ask the questions, interrogate the sources, or consider the ways of knowing that are common currency in the history of science before 1900 is compounded by the failure to study the sites and objects of natural history. For example, the question of authority and validation in the case of bioprospecting is made more challenging to answer by the absence of solid twentieth-century work on the botanic(al) garden. Botanic gardens are the topic of intricate and valuable studies that show them to be crucial junctures of political, colonial, professional, social, and scientific ambition across the

[40] Peter Galison, "Ten Problems in History and Philosophy of Science," *Isis*, 2008, *99*:111–124.

[41] Abena Dove Osseo-Asare, "Bioprospecting and Resistance: Transforming Poisoned Arrows into Strophantin Pills in Colonial Gold Coast, 1885–1922," *Soc. Hist. Med.*, 2008, *21*:269–290; Shane Greene, "Indigenous People Incorporated? Culture as Politics, Culture as Property in Pharmaceutical Bioprospecting," *Current Anthropology*, 2004, *45*:211–237; and John Merson, "Bio-prospecting or Bio-piracy: Intellectual Property Rights and Biodiversity in a Colonial and Postcolonial Context," *Osiris*, 2000, N.S., *15*:282–296.

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eighteenth and nineteenth centuries.[42] Perhaps it is because they are so strongly conceptualized as Victorian monuments that their presence and activity in the twentieth century seem anachronistic or insignificant.[43] But this is an error. Botanic gardens and related specimen collections are intimately connected to DNA databases, to genetic science, and—still—to bioprospecting; after all, one way of avoiding awkward legal challenges relating to the ownership of biological specimens is to source them from your in-house university garden. A recent analysis by a New Zealand ecologist suggested that nineteen of the thirty-four most dangerous invasive plants (according to the International Union for the Conservation of Nature) "escaped" into ecosystems from botanic gardens, many of which were themselves legacies of colonial activity by European nations; this seems to pose provocative questions about indigenous identity, postcolonial reconstruction, even environmental history.[44] The typical sites of natural history are still cruxes of political, social, and scientific activity and therefore deserve our attention.

Such sites are also important spaces in the history of the nonbiological sciences. In 1954 a UNESCO-funded study was published that gives the details of all the high-altitude research sites in the world.[45] Forty-four sites were listed, nearly all of which had been built in the twentieth century. These are complex scientific spaces, funded by governments, military organizations, overseas agencies, charities, and universities. They are often astronomical or meteorological, but frequently astronomers and meteorologists share space with biologists, veterinarians, doctors, and physiologists. We begin to know a little about these spaces when they are observatories, but the mountain-as-research-site is much better understood in the nineteenth century than the twentieth century.[46] Such nineteenth-century work is sometimes a crucial foil for questions about the twentieth century: take the concept of the "personal equation" in astronomy (whether we want to see it as a problem internal to physics or a problem crossing the physical and human sciences), where the challenge of understanding and regulating individual human variation in measurement and observation became crucial to the fine judgments necessary for certain sorts of astronomical observation.[47] Such personal variation was significant even at sea level, and one can assume that with the building of mid- and high-altitude sites the effect of oxygen depletion must also have an impact on fine motor skills, reaction time, and general cognition.

Although schemes to introduce supplemental oxygen into laboratory workspaces (or provide individual respiratory assistance) were proposed for high-altitude sites, these do

[42] See, e.g., Richard Drayton, *Nature's Government: Science, British Imperialism, and the Improvement of the World* (New Haven, Conn.: Yale Univ. Press, 2000).

[43] Unless, that is, they are the factory/technoscience sources of genetically standardized plants for molecular biological work. "Monsanto . . . is like the Kew Gardens of the nineteenth century: a metropolitan 'center of calculation'": Sheila Jasanoff, "Biotechnology and Empire: The Global Power of Seeds and Science," *Osiris*, 2006, N.S., *21*:273–292.

[44] P. E. Hulme, "Addressing the Threat to Biodiversity from Botanic Gardens," *Trends in Ecology and Evolution*, 2011, *26*:168–174.

[45] Serge A. Korff, ed., *The World's High Altitude Research Stations* (New York: UNESCO, 1954).

[46] Simon Schaffer, "The Shape of the Mountain" (unpublished MS, 2009); Charlotte Bigg, David Aubin, and Philipp Felsch, "Introduction: The Laboratory of Nature—Science in the Mountains," *Sci. Context*, 2009, *22*:311–321; Deborah R. Coen, "The Storm Lab: Meteorology in the Austrian Alps," *ibid.*, pp. 463–486; Felsch, "Mountains of Sublimity, Mountains of Fatigue" (cit. n. 8); and Le Gars and Aubin, "Elusive Placelessness of the Mont-Blanc Observatory (1893–1909)" (cit. n. 8).

[47] Simon Schaffer, "Astronomers Mark Time: Discipline and the Personal Equation," *Sci. Context*, 1988, *2*:115–145; and Jimena Canales, "Exit the Frog, Enter the Human: Physiology and Experimental Psychology in Nineteenth-Century Astronomy," *Brit. J. Hist. Sci.*, 2001, *34*:173–197.

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not seem to have been realized until the turn of the twenty-first century.[48] On the surface this seems to be a puzzle—we at least have to explain how an obsession with minimizing human variation and physical inadequacy, part of the drive for mechanization and precision that dominates our understanding of nineteenth-century physics, is deprioritized in the twentieth century.[49] Maybe the mechanization had reached a point where it was thought that human failings could no longer significantly interfere with the results? Maybe the technology of oxygen supplementation was just too expensive or inefficient to be worth installing? Maybe modern astronomers resisted intervention because of a macho sensibility that rejected "artificial" aids?[50] Perhaps the sort of researcher who seeks work in these difficult scientific spaces tends to self-identify as an explorer, outdoors enthusiast, or adventurer and is as likely to relish the physically challenging aspects of his or her work as to want to ameliorate them? Perhaps combinations of these factors and specific local circumstances prevented observatory managers from introducing oxygen systems?[51] Or might we be starting from a false assumption; could we be wrong in our interpretation of the nineteenth-century material?

The concept of the scientist as hero or explorer has not disappeared in the historiography of the twentieth century, but we tend to take these as metaphorical concepts, discussing political resistance, intellectual bravery, and self-experiment instead of physical encounters with the dangerous world outside the laboratory.[52] Again, I would not suggest that the metaphorical explorer is not important but, rather, that this metaphorical sort of exploration and its related concept of heroism exist alongside the very real physical exploration of a very high, very hot, very cold, and very dangerous material world by men—and, extraordinarily rarely, women (further, as Naomi Oreskes has clearly shown, women's work in this area can often be redefined as "drudgery" rather than "heroism").[53] As well as the standard barriers, such as access to scientific training, the financial demands of expeditions, and institutional sexism, women have been deliberately excluded from many expeditionary activities on principle, as they were thought to disrupt group dynamics and introduce "unnecessary complications" into research; it is only with the late 1960s, possibly influenced by space research and the opening of extreme leisure spaces, that women appear in any numbers at all as participants in scientific explorations to extreme environments.

If we stay with astronomy as our example, then the end of the twentieth century, and the beginning of the twenty-first, has produced some extraordinarily strange research spaces. At the time of writing the largest "telescope" in the world is not up a mountain but, rather, in Antarctica: literally *in* Antarctica—it is the IceCube Neutrino Observatory based at the Amundsen-Scott South Pole station, where holes thousands of meters deep have

[48] J. B. West, "Oxygen Enrichment of Room Air to Relieve the Hypoxia of High Altitude," *Respiration Physiology*, 1995, *99*:225–232.

[49] As opposed, of course, to using the sensitivity of the human body as a method of measurement; see Dettelbach, "Stimulations of Travel (cit. n. 8).

[50] My thanks to Simon Schaffer for this suggestion.

[51] Some discussion of these questions, regarding the Keck Observatory, can be found in the special collection of materials John West has gathered at the University of California, San Diego: John West Papers, Mandeville Special Collections Library, University of California, San Diego, Box 4/File 14 and Box 20/various.

[52] Another figure is the scientist as radical; see David Kaiser, *How the Hippies Saved Physics* (New York: Norton, 2012). On the complexities of the (anti)heroic identity and exploration see Mary Louise Pratt, *Imperial Eyes: Travel Writing and Transculturation* (London: Routledge, 1992).

[53] Naomi Oreskes, "Objectivity or Heroism? On the Invisibility of Women in Science," *Osiris*, 1996, N.S., *11*:87–113.

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been drilled across approximately a cubic kilometer of the Antarctic ice. Sensors strung down these holes are used for the detection of neutrinos. It is an enormous laboratory and simultaneously an enormous field site and simultaneously an enormous instrument, with both local and distant users. The on-site scientists and engineers are working in one of the most inhospitable environments on earth, alongside teams of scientists engaged in other projects, military personnel, visitors, and sometimes sportspeople, explorers, and television crews. Without an active understanding of exploration in the service of science, and of the scientist-explorer, it is hard to see how we will be able to contextualize the knowledge produced in sites such as this.[54]

What's more, the explicitly transnational web of relationships that makes up the exploration and extreme travel community now provides a superstructure that we must take into account in order to understand the ways in which modern science is funded and practiced. We have clear conceptual space for the amateur scientist in the nineteenth century and should make new space for the "citizen science" of the late twentieth and twenty-first centuries; exploration has a significant impact on this activity, as scientists literally rely on the hobbies of other people to fund their research and to gather and interpret data.[55] Perhaps the largest example is Adventurers and Scientists for Conservation (ASC), a North American–based organization that arranges partnerships between "adventure athletes" and researchers in need of data from remote places. The organization specifically emphasizes that its athletes are volunteers, who adopt the identity of "adventurer-scientists" because they have a "strong desire to make more of their expeditions."[56] Here, "doing science" is represented as a way to add moral as well as social and economic value to a leisure pursuit.

ASC is just one example among many of organizations that use the contributions and (usually voluntary) work of people other than professional scientists.[57] While such organizations often make explicit appeals to the "good old days" of amateur Victorian scientists and explorers, this is a form of nostalgia rather than an accurate representation of either modern citizen science or twenty-first-century field science, both of which we need to understand fully as modern phenomena.[58] This essay, with its many unanswered questions, is an attempt to outline the sorts of histories we are going to need in order to understand a growing and crucial area of contemporary science, which will affect everything from bird-watching to interstellar travel.

[54] Even if that is to understand how the relationship between science and exploratory identities has changed, as current researchers suggest that the relative ease of access to Antarctica in the twenty-first century means that it can now appeal to people who "just" want to do the science, rather than those with a specific interest in experiencing the Antarctic. Scott Polar Research Symposium, "Issues of Historical Practice in the Polar Regions," Cambridge, 29 Mar. 2011.

[55] A classic example treating the work of nineteenth-century amateur scientists is Anne Secord, "Science in the Pub: Artisan Botanists in Early Nineteenth-Century Lancashire," *Hist. Sci.*, 1994, *32*:269–315. See also Allan Chapman, *The Victorian Amateur Astronomer: Independent Astronomical Research in Britain, 1820–1920* (New York: Wiley, 1999); and Samuel J. M. M. Alberti, "Amateurs and Professionals in One County: Biology and Natural History in Late Victorian Yorkshire," *J. Hist. Biol.*, 2001, *1*:115–147.

[56] http://www.adventureandscience.org/about-us.html (accessed July 2012). My thanks to the referee who suggested the Audubon Society's Bird Count and the Encyclopedia of Life as other examples.

[57] A large proportion of "citizen science" projects consist of computer-based tasks farmed out to hundreds, or thousands, of participants, but many also include physical activities and exploration: aside from Adventurers and Scientists for Conservation there is, for example, also a biological survey scheme for divers called Earth Dive, http://www.earthdive.com/site/aboutus/default.asp (accessed Feb. 2013); as well as more homely projects such as the Royal Society for the Protection of Birds annual garden bird survey in the United Kingdom, http://www.rspb.org.uk/birdwatch/ (accessed Jan. 2013).

[58] Driver, *Geography Militant* (cit. n. 8).

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Exploration is always a consumer of science; and it is frequently a producer of science. While it may not be in and of itself "a science," it may well be a "way of knowing," and it is undoubtedly a constitutive part of modern science. So too are analytical and savant science, museum and natural history ways of knowing, amateur participation, and travel narratives, not just in the sixteenth, seventeenth, eighteenth, and nineteenth centuries, but also—and maybe even more so—in the twentieth and twenty-first.

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